as a criticism of the authors—these are assumptions commonly made in the course of testing economic hypotheses—but to call attention to the crudeness of our measurement techniques.

The third difficulty in testing the induced innovation hypothesis is related to the second. It is again the problem of distinguishing between choices within the existing production technology and development of new technology. The source of the difficulty this time arises from changes in the level or scale of production. As production expands, a different, more appropriate method of production may be chosen from among the existing ones. The chosen method may, for example, be more capital-intensive than the best method for producing at lower quantities. The shift to the different method of production results in a change in the optimal factor ratios even with relative factor prices fixed. Thus, what may appear to be an induced technical advance is in fact not. In technical terms this difficulty is avoided by assuming that the production function is homothetic. But then this assumption requires some independent justification.

The fourth difficulty is associated with the possibility that the costs of achieving different types of technical advances differ. Although it is reasonable to assume that if all other things are equal a technical advance that economizes on the relatively more expensive factor of production is the more profitable, in reality other things might not be equal. It might in fact be more profitable to economize on the cheaper factor of production because that type of technical advance is easy to come by. Thus, a test of the induced technical advance hypothesis requires that the costs of achieving different technical advances be made comparable.

The authors deal with each of these difficulties to a greater or lesser extent. Their studies of changes in agricultural technologies in the United States, the United Kingdom, France, Germany, Denmark, and Japan between 1880 and 1960 on the whole support the induced innovation hypothesis. They claim that starting with essentially the same technologies in 1880 the United States and Great Britain experienced laborsaving technical advance while the other countries experienced neutral technical advance. There are some anomalies, however. At different periods of time technical advance tended to be laborsaving in Japan, Great Britain, France, and Denmark despite decreases in the price of labor relative to the price of land. A more intensive study of agricultural technology in the United States between 1912 and 1968 indicates, according to the authors, that "the biases of technical change with respect to machinery, labor, and fertilizers have been responsive to changes in relative factor prices." They claim in addition the presence of an external cost bias favoring machine-using technical advances to explain the prevalence of this type of technical advance despite an increase in the price of machines relative to other factors of production.

Beyond the tests of the induced technical advance hypothesis the authors raise several noteworthy points. The first relates to the dilemma confronted by less developed countries seeking to adopt technologies from the more developed countries. Less developed countries typically have a relatively large agricultural sector. This would suggest that adoption of modern agricultural methods might be the most beneficial, at least initially. As productivity in this sector improves, the price of agricultural commodities falls. The demand for agricultural commodities, however, is relatively inelastic and increases less than proportionately, thereby reducing the demand for labor in this sector. Thus, the less developed country adopting this strategy experiences unemployment unless it has a growing industrial sector to absorb the labor released from the agricultural sector. This helps explain the eagerness of less developed countries to industrialize even when they can purchase industrial products from abroad more cheaply than they can produce them domestically. The adoption of modern industrial technologies has its drawbacks, too, because it tends to be heavily capital-intensive whereas labor-intensive methods would be most suitable for the less developed country.

The second point, mentioned earlier, is the authors' contention that the induced technical advance hypothesis relates to applied research and not basic research. The motivations for the latter type of research, they claim, are varied. Yet apart from the difficulty of distinguishing between applied and basic research, the two tend to be highly interactive, and there is the question of what is meant by economic motivation. If by economic motivation it is meant that a research activity requires a commitment of money and is done for a monetary reward then both basic and applied research are so motivated. It's just that the form of payment may be different in the two cases. And we in the universities, the primary source of basic research, know that shifts in government funding of research creates concomitant shifts in the focus of research in both the short and the long run.

This book is the product of many years of work. Some of the chapters have appeared as journal articles. Bringing them together provides a culmination of this work that is larger than the sum of the parts. This book is not the last or conclusive test of the induced technical advance hypothesis. More studies will follow. Each of them, however, will have to take this book into account for its substantive and methodological contributions to the subject.

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Geochemistry

Terrestrial Rare Gases. Proceedings of a seminar, Hakone National Park, Japan, June 1977. E. C. ALEXANDER, JR., and M. OZIMA, Eds. Center for Academic Publications Japan and Japan Scientific Societies Press, Tokyo, 1978 (distributor, Business Center for Academic Societies Japan, Tokyo). xii, 230 pp., illus. $24.50. Advances in Earth and Planetary Sciences 3.

Rare gases are useful in geochemistry and cosmochemistry because their scarcity permits sensitive detection of nuclear processes occurring in nature with rare gas products and their nonreactivity provides a broad array of tracers for studying, almost without chemical complications, physical processes in nature. Starting with Rutherford, who invented uranium-helium dating, which was the first radiometric clock, and continuing to and beyond the banner year 1969, when the store of important samples was vastly enriched by returned lunar rocks and by the Allende meteorite fall, rare gas studies have contributed a surprisingly valuable subchapter in cosmochemistry and radiometric dating. Relatively neglected by comparison has been the use of rare gas isotopes as tracers in the investigation of the evolution of the earth and atmosphere, where nongaseous tracers such as radiogenic lead, strontium, and neodymium have received more of the play. It was this circumstance that prompted Alexander and Ozima to organize a U.S.-Japan seminar on rare gases and to edit the slim, handsome volume that is the proceedings of the seminar.

In 1969, Clarke, Beg, and Craig in the United States detected excess ^{3}He in sea water and Mamyrin, Tolstikhin, Anufriev, and Kamenskii in the Soviet Union detected excess ^{3}He in volcanic gases. A year later, Bennet and Manuel unequivocally confirmed previously weak evidence for an excess of ^{129}Xe (from decay of the extinct radioactivity ^{129}I) in CO_2 from a gas well in New Mexico. It was clear almost immediately from each of these observations that outgassing of a juvenile gaseous component residing within the earth persists visibly until the present. The successful and energetic development of helium isotopic studies by Craig and co-workers in America and by Tolstikhin and co-workers in Russia has been the most exciting aspect of the modern work in terrestrial rare gases, and the book contains good papers by Craig and co-workers and by Tolstikhin that exhibit interesting geographical patterns in the efflux of the primordial gas. Tolstikhin, who unfortunately was unable to attend the seminar, has nevertheless contributed a fine review of the latest Russian work. That work had previously been of only limited accessibility to scientists who don't read Russian. As reviewed by Tolstikhin the work is impressive both in scope and quality; his review will justify owning the book for many.

The majority of space in the book is devoted to outgassing models for the earth and to the utilization of argon and helium data for the mantle to fix parameters in such models. The papers by Tolstikhin, Manuel, Bernatowicz and Podosek, Hamano and Ozima, Fisher, Schwartzman, and Hart and Hogan all deal with this subject, at least in part. I wish I could say that some sort of helpful consensus emerges from all that writing, but it does not. Unevenness in the quality of the papers is one of the difficulties, but a more basic problem, expressed clearly in the long but readable review by Bernatowicz and Podosek, is that input parameters for the models, such as the potassium content and the ratio $^{40}Ar/^{36}Ar$ for the mantle, are not sufficiently well known to constrain models usefully. We are just at the point where we can identify juvenile outgassing from the mantle, but we do not yet have measurements of the isotopic composition of rare gases that we can be certain are representative of the mantle as a whole, or even regionally.

In short, the book is a useful glimpse of what is happening in this field and will be valuable to specialists. Those outside the field who wish to make use of its fruits would best bide their time. The papers in the book are enticing, but the payoff is not yet at hand.

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Fossil Communities

The Ecology of Fossils. An Illustrated Guide. W. S. McKERROW, Ed. MIT Press, Cambridge, Mass., 1978. 384 pp., illus. $22.50.

Four decades ago, Maxim K. Elias published in the *Geological Society of America Bulletin* a diagram showing a comparison of fossil animal and plant associations in the Permian "Big Blue" deposits of Kansas with modern shallow marine benthic communities. This diagram set forth the way of explaining the fossil record that, continuing with A. M. Ziegler's highly influential papers on Silurian communities beginning in *Nature* in 1965, culminates in the present book exemplifying the "community approach" to paleoecology.

The essence of *The Ecology of Fossils* is a series of 125 black-and-white block diagrams (plus a page or so of text for each) showing assemblages of fossil animals on the ocean bottom around Great Britain from the Cambrian to the present. The breadth of coverage over geologic time within one publication is unique and provides the main fascination of the book. Although some of the "snapshots" may not conform entirely to reality, there appear to be no time-dependent systematic biases that would interfere with several general conclusions to be drawn from the book. As I will explain, the errors in the figures are of the sort that exist when a photograph labeled "The Family" shows a local family standing together on the front porch and also includes a couple of in-laws who happened to be visiting from out of town.

As other reviewers of this book have documented, some fossils shown together in some diagrams in fact are not normally fossilized in the same beds. In addition, the crowding together of fossils in the reconstructions is sort of like having all of the family line up on the porch—they don't normally occur that way, alive or dead. A judgment based on a particular taxon in a particular figure could therefore be in error, although in the absence of time-dependent systematic errors such properties as the size of the family group, the general dress and appearance, and the functional morphology reasonably can be inferred. Thus my basic judgment of the book is that it is most interesting, reliable, and useful as a general book to show what one should and can look for in fossil assemblages rather than as a specialist's handbook of facts correct in every detail.

One question the reader may have is whether the communities are "representative" of those that actually existed during the Phanerozoic. To investigate this, I compiled Table 1, in which each of the 98 marine assemblages is placed in one of seven categories according to substrate. Of the assemblages in Table 1, approximately 45 percent would be considered to be in limestones, 15 percent in sandstones, and 40 percent in shales. In the geologic record as a whole, approximately 20 percent of the rocks are limestones, 30 percent are sandstones, and 50 percent are shales. Thus the biological assemblages shown in McKerrow's book would have too little representation of assemblages from shales and too large a representation from limestones (especially reefs) relative to what is found in the geologic record as a whole. Whether or not the assemblages are proportionately representative of rock types of Great Britain, which is the source of nearly all the assemblages, is not known.

We can also ask how representative biologically are the assemblages in this book with respect to the original assemblages. A few generalizations seem to apply.

Table 1. The 98 marine assemblages of *The Ecology of Fossils* categorized by geologic age and by substrate in which they occur.

Geologic age	Number of assemblages							
	Terrigenous mud	Algal mud	Terrigenous sand	Coral-algal-shell sand	Reef	Hard ground	Basin	Total
Cambrian	3						1	4
Ordovician	7	2	1	1			2	13
Silurian	5		3	2	1		2	13
Devonian	2		1	1			1	5
Carboniferous	2	2		5	4		2	15
Permian				1	1			2
Triassic								0
Jurassic	5	4	2	7	2	3	1	24
Cretaceous	6		2	4		3		15
Cenozoic	2		5					7
Total	32	8	14	21	8	6	9	98